Interactive comment on "Qualified temperature, salinity and dissolved oxygen climatologies in a changing Adriatic Sea" by M. Lipizer et al.

Anonymous Referee #2

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1 General comments

The paper presents a new regional climatology of the Adriatic Sea for T, S and dissolved oxygen for the 1911-2009 period, using an inverse method. Overall, the overall presentation is well-structured and the language fluent and precise, but some parts of the paper should be clarified (see next Section).

While the introduction is clear and relevant objectives are properly defined, the result description is difficult to follow for readers not familiar with the regional oceanography, and relies on figures with an insufficient quality.

As a new climatology, it should be compared more thoroughly with other climatologies (following p 348, l 14), either global (even if their resolution would probably be insufficient) or regional (MedSea or Adriatic) products. In particular, it would be relevant to identify the differences/improvements with other climatologies due to:

- *the use of a extended data set (longer period, maybe more historical data sources than previous attempts to create a climatology),*
- *the use of a particular method of interpolation.*

More details about the interpolation method (about one paragraph) would be useful. With the description in Section 2.3, it is not clear to me how the method works. Section 3.2 provides a description of the gridded maps obtained for the different variables, at different depths and for different seasons. Though I see this description useful, my concern is that for readers not familiar with the Adriatic Sea, the interest is not obvious, as it is not clear whether there are modifications or improvement with respect to previous climatologies.

Finally, the core of the work (a set of new climatological fields) deserves more diffusion, so I believe the authors should provide not only figures (also see comments in the next Section), but also the gridded fields themselves (NetCDF format seems adapted for this).

> **Authors:** Following the comments and suggestions of the referee, the manuscript has been partially modified. A more detailed comparisons with older climatologies, highlighting the new outcomes of this analysis (e.g. the patterns observed in the deeper layers, the "uncertainty" in terms of data-approximation misfit associated to the climatological maps, the long term variability in the deep water properties) has been added, as well as more information on the approximation method and on the computation of the error field (see detailed answers below the Specific comments) have been provided. Most figures have been redrawn and the NetCDF are available through: http://nodc.ogs.trieste.it/nodc/metadata/doi.

2 Specific comments

2.1 Text

p 332, l 25: A more accurate definition of "climatology" could be given.

> **Authors:** The text has been modified accordingly.

p 333, l 1: do numerical models always rely on climatologies for initial conditions? Not on another numerical model with a larger spatial coverage?

Authors: Numerical models can be initialized both by climatologies (e.g. Halliwell et al., 2008) and coarse models. Both choices have pros- and cons. Using a coarse resolution model can lead to unrealistic initial conditions in coastal and shelf areas, where the resolution cannot properly resolve the relevant processes and the boundary conditions are less detailed.

Halliwell Jr, G. R., Shay, L. K., Jacob, S. D., Smedstad, O. M., & Uhlhorn, E. W. (2008). Improving ocean model initialization for coupled tropical cyclone forecast models using GODAE nowcasts. Monthly Weather Review, 136(7).

p 333, l 15: "which are regarded as providing the most suitable records of possible long-term changes" ! can you justify it? Also (and maybe it comes later in the text, but the data scarcity in the deepest layer con constitute an obstacle for studying the long-term changes.

Authors: We acknowledge the comment and this part has been modified and an explanation has been added. The revised version provides a modified text, with new references. The deepest parts of the basin are considered to act as an integrator of signals coming from different processes acting in the basin (i.e. continental shelf pump, deep convection and lateral exchanges) that are supposed to respond to climate variability, as recently detected for the last 13.000 years in the sedimentary records collected in the South Adriatic Pit (Siani et al., 2013).

Siani, G., Magny, M., Paterne, M., Debret, M., & Fontugne, M. (2013). Paleohydrology reconstruction and Holocene climate variability in the South Adriatic Sea. Climate of the Past, 9(1).

p 333, l 26: they are some references to previous work on climatologies, and it would be relevant to know which interpolation/gridding methods were used for these. Also, what about the global climatologies, such as the series of World Ocean Atlas (http://www.nodc.noaa.gov/OC5/woa13/) or CORA (http://www.coriolis.eu.org/Allnews/News/CORA3.4-gridded-fields)?

Authors: All previous climatologies for the Adriatic were based on objective analysis (this has been added in the text). The global climatologies mentioned are also based on OA and have a much lower spatial resolution (1°) compared to this study (1/16°).

p 334, l 20: additional plots are available on OGS-NODC web site. ! it would be nice to have an url to access the fields as well.

Authors: It has been added and NetCDF files area available online: http://nodc.ogs.trieste.it/nodc/metadata/doi.

p 337, l 14-15: Quality Control (QC) procedures (Giorgetti et al., 2005; Holdsworth, 2010; SeaDataNet, 2010) which guarantee the consistency of the merged data ! I understand that QC improves the quality of the whole dataset, but it is not clear to me that QC can guarantee the consistency.

Authors: This sentence has been modified.

p 338, l 3-5: concerning the data selection according to depth: how to you deal with possible different vertical resolution of the profiles? Since you select the data in a layer around the depth of interest, you might get more data at locations where the profiles have a fine resolution. Secondly, imagine you have a profile with observations at 340 and 360 m, but no observation between these depths. Does this means that for this profile, for the 350 m layer, you don't have any data?

Authors: Since 1975 most data were acquired with profiling CTD probes equipped with oxygen sensor, which provide data with 1 meter vertical resolution. Before the use of profiling automatic probes, "standards depths" were commonly sampled (LEVITUS94) and most levels produced in this study coincide with the "standard depths". The depth levels discussed in the article are all referred to "standard depths". For datasets compiled earlier than 1975, in case of absent data, no interpolation among proximal observations has been performed.

LEVITUS94: World Ocean Atlas 1994

p 338, l 18: [Bretherton et al., 1976] did not use finite-element method.

Authors: This quotation has been replaced with: Brasseur et al., 1996

p 339, l 2: Finally, OI implementation must consider all the data at the same time ! Several OI implementations do not consider all the data points, but rather the data located within a circle (with a determined radius) around the point of interest. See for example Menemenlis et al. (1997), http://onlinelibrary.wiley.com/doi/10.1029/97JC00697/abstract

Authors: The suggested reference has been used and this part has been modified accordingly.

p 339, l 4: can you define what are the "nodes"?

Authors: nodes have been defined in section 2.3.

p 339, l 8: the way the error field is computed is merely addressed in the paper. See also Beckers et al. (2014) http://journals.ametsoc.org/doi/abs/10.1175/JTECH-D-13-00130.1

Authors: This part has been added in the Appendix with a detailed description of the theoretical aspects.

p 339, l 28: Ordinary Cross Validation: the referenced paper is not the most adequate.

Authors: This quotation has been replaced with: Stone, M. (1974). Cross-validatory choice and assessment of statistical predictions. Journal of the Royal Statistical Society. Series B (Methodological), 111-147.

p 340, l 14: can you quantify the number of data points that are repeated on common locations?

Authors: The number of data points repeated on common locations varies according to the in situ data availability. This number ranges between a maximum of 94 data in the Northern Adriatic, where spatial data coverage is particularly high, to a minimum of 1 datum in some areas of the southern basin.

p 341, l 5 : Climatology-Observations Misfit: again, it would be useful to know how many times you have various observations at the same location. Do you allow for a deviation from the actual position?

Authors: As reported in the previous answer, the number of data on common locations varies. The Climatology-Observations Misfit has been calculated as the average of the difference between all in situ data (*d*) lying within the box (or 1/16°resolution) defined by the grid generated by DIVA and the corresponding approximated field (*y*). The box size is constant throughout the domain, but data coverage is not uniform (see previous comment).

Then what happens in cases (most frequent) where you have only one observation at the same location? The lowest value for COM would be obtained when di = yi, i.e., when the observation is equal to the approximated field. In this case, you are with a strict interpolation.

Authors: The case of "*di = yi, i.e.,* when the observation is equal to the approximated field" is unrealistic, since the formulation of the cost function J(φ) defined as:

$$J[\varphi]=\sum_{i=1}^{Nd}\mu_i L^2\left(d_i-\varphi(x_i,y_i)\right)^2+\int_D(\nabla\nabla\varphi:\nabla\nabla\varphi+\alpha_1 L^2\nabla\varphi\cdot\nabla\varphi+\alpha_0 L^4\varphi^2)\,dD \qquad (1)$$

(for details see appendix A) in constituted by two terms: the first is a weighted squared norm of the observation-approximation misfit (which can be zero in case of coincidence of approximated value with the observation) and a second term which is an integral of positive (squared) quantities weighted by non-zero parameters

- α0,(anomalies minimization) (usually assumed as 1)
- α1,(trends minimization)…

• α2,(second order regularization)…

….

This means that the second term may be zero when the three terms under the integral identically vanish, which is definitively unrealistic case.

So before discussing the COM, the reader would need to know which analysis parameters are selected prior the analysis (what is explained in the appendix).

Authors: The parameters have been presented in section 2.3.

p 341: The noise is globally retained in the approximation process by selecting . . .! this paragraph may better fit in Section 2.3, where the method is described. Also, the whole paragraph is not clear to me. For instance, "a weight by DIVA solver for the minimization of the cost function" does really make sense without further context.

Authors: The comment is appropriate. This part has been moved to section 2.3.

p 342, l 9: depends on the data coverage only . . .! and what about the noise associated with observations, as stated previously in the text.

Authors: This sentence has been modified. The noise contribution is included in the variance of the observations.

p 344, l 3: section 3.1: I would expect to have the values of the analysis parameters (see p 339, l 9-11) used for the interpolation prior to the results, even if the procedure to estimate them is provided in the appendix.

Authors: This has been modified and section 2.3 includes now the analysis parameters.

p 346, l 24: which season?

Authors: In winter. It has been better specified.

p 360, 1st conclusion: it's clear that a new climatology has been created, but the improvements and differences with respect to previous work are not discussed at all.

Authors: This part has been improved in the discussion of the results. The presentation of the results now focuses on the new outcomes of this study in comparison with previous climatologies.

2nd conclusion: on what is based this affirmation?

Authors: We acknowledge the comment and this has been removed from the conclusions.

p 363; eq A1: what stand di, Nd, L, . . . for?

Authors: We agree with the observation. All the variables have been properly defined.

p 364, l 3: The correlation length (L) is the parameter that defines the dimensions of the grid ! which grid?

Authors: The Variational Inverse Method (VIM) is used to solve an approximation problem by using a Finite Element Method (FEM). This popular technique produces a solution by connecting local solutions evaluated on connected triangular domains that constitute the FEM grid. As a rule of thumb, it is safe to relate the density of the grid nodes in function of the correlation length: the lower the correlation length, the higher the node density. To have a proper approximation of the spatial dynamics, L/3 should be assumed as a typical scale of the grid element. This will guarantee that the dominant scales of spatial variability (captured by the correlation function) are properly resolved.

This part has been added and clarified in the method description.

p 364, l 4: every grid element is the combination of three sub-elements ! could you be more explicit?

Authors: See above. The text has been integrated with a short explanation.

p 364, l 4: To avoid sub-sampling, it is required that L/3 is smaller than the scale of the dominant processes analysed. ! what is meant by sub-sampling? And why L/3 (why not L?) has to be smaller than the characteristic scale?

Authors: See above. The text has been accordingly modified.

p 364, l 15: (L = 0.8 for physical parameters and 1.5 for DO) ! this information should be present in the core of the text, Section 2.3 for example.

Authors: The text has been accordingly modified and correlation length and signal-to-noise ratio have been indicated in method section 2.3.

p 364, l 36: The correlation length and the signal-to-noise ratio are, therefore, directly comparable to the corresponding parameters in OI (Troupin et al., 2012). ! what is the relation with the rest of the paragraph?

Authors: This sentence has been moved to section 2.3.

2.2 Figures

Figure 2: hard to see without zooming in the pdf. It is obvious that more measurements are available, for instance, in the 1990's than in the 1960's. Is that unequal distribution taken into account when you compute the climatological gridded fields?

Authors: The figure has been re-drawn. The climatological gridded fields are built using all available in situ measurements. The seasonal climatological maps are based on a wide time - frame (1911 - 2009) which is considered large enough to compensate for the unequal distribution of data, i.e. gaps of data in some years, as seasonal data availability is sufficient throughout the study period (see New Fig. 2b-d).

Figure 3: looking at panels c) and d) gives the impression that the density anomaly was increased (more dots above 29 kg/m3 in d). Can you explain that? Also, the diagrams would be more readable if the axis limits were better chosen (for example, the y axis goes until 18_C, while the max. temperature seems closer to 15_C. Another suggestion to make the comparison easier would be to create only 2 panels, one for summer 100 m and one for winter 200 m, using different colors for in situ and reconstructed.

Authors: The suggestion has been followed and new figures have been provided. The comparison among in situ and approximated data indeed shows a density decrease (of about -0.15 kg/m3) in the approximated values, with respect of the core of in situ data. The new Figure 3, however, shows also the large dispersion of in situ data (both at 100 m and 200 m), therefore the slight shift towards lower density in the approximated values is driven by the quite consistent number of low-density experimental data, particularly evident at 100 m (see attached new Fig. 3).

New Fig. 3: Comparison of TS diagrams based on unevenly distributed in situ observations (black) and approximated data on a regular 1/16° grid (red) in summer at 100 m (left) and in winter at 200 m (right).

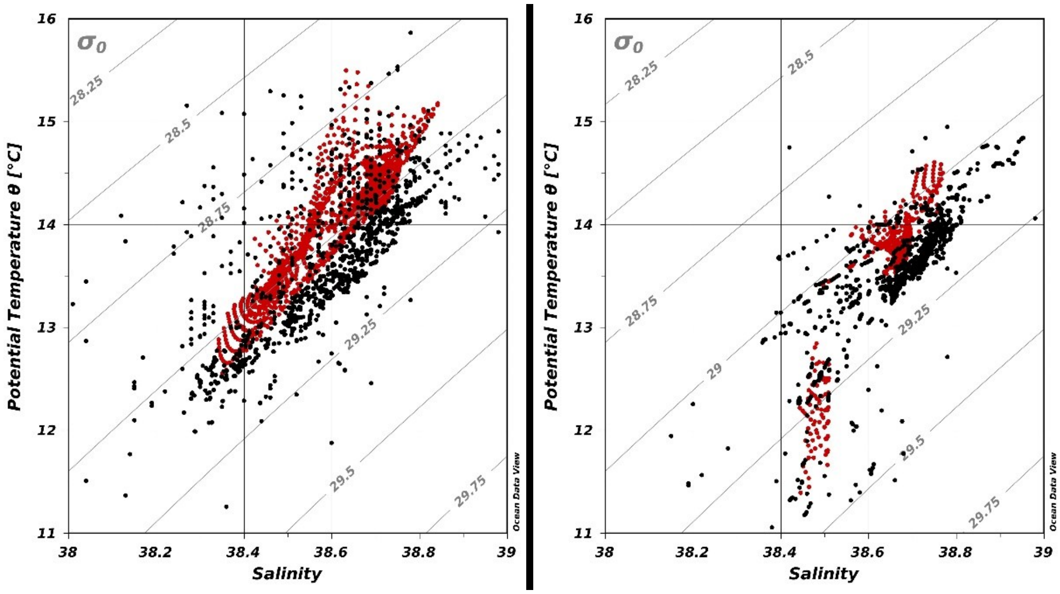

Figure 5: the overall quality of these figures is below the rest (aliasing, resolution, . . .). The labels attached to the isolines are hardly readable, and for some of the sub-figures, it is hard to relate a given color to the colorbar. In addition, it would be more relevant to have the sub-figures corresponding to a common variable (T, S or Oxy) together, in order to easily identify the seasonal cycle mentioned in the text. As this figure 5 seems to represent a central result of the paper, I would recommend the authors to re-prepare it with a better quality.

Authors: Figure 5 has been re-prepared accordingly and sub-figures corresponding to a common variable (T, S or Oxy) are now presented together, in order to easily identify the seasonal cycle mentioned in the text.

Figure 6: the labels (x and y axis, colorbar) are hardly visible

Authors: Figure 6 has been re-prepared accordingly.

3 Minor comments

p 339, l 9: The methods only requires ! method

Authors: corrected

p 347, l 9: by inversion ! by an inversion

Authors: corrected

p 363: finite element ! finite-element

Authors: Both forms are acceptable but “finite element” is more widely used.

p 364: SNR ratio ! remove "ratio" (redundant)

Authors: It has been modified.